

called five or six years later to see one of these children who had a well-marked second attack of measles. The parents were congratulating themselves at the absence of the other two, at boarding-school, when at the very time of the visit one of these latter was sent home, also suffering from measles. How rapidly contagion is sometimes communicated he illustrated by another case. A mother, entering the Tuileries with her child, on hearing an infant near her suffering under a paroxysm of pertussis, at once withdrew. Nevertheless, at the end of some days her child was seized with pertussis. M. Empis considered the admission of the spontaneous development of the eruptive fevers to be a mere gratuitous hypothesis, which may have a mischievous effect by diminishing circumspection in regard to preservative measures. The true mode of propagation of these affections is by infection, to which in populous localities there is constant exposure. M. Roger explained that no one would think of contesting the contagion of the eruptive fevers. All that is now advanced is that we may admit a kind of spontaneous development when contagion has not been in operation. M. Chauffard observed that, while it cannot be doubted that certain diseases were originally imported from distant regions, yet, once brought here, may they not become in some sort naturalized, and be liable to spontaneous development? In 1855 there had been numerous cases of cholera in Paris without any such appearing in the provinces, when, on June 7, the same day on which the first case was observed at Avignon, the disease appeared at several distant points. Here spontaneous production is a more natural explanation than infection; and do we not see the same thing occurring daily with respect to variola, rubeola, and scarlatina? M. Simonet replied that a geographical progress has always been attributed to cholera. Its propagation may thus be traced, and its spontaneous origin here cannot be admitted. As to the eruptive fevers, children are constantly taken to the promenades, and how are we to ascertain when and how they have contracted the contagion? M. Guérard remarked that we have constantly the opportunity of observing that the most infectious diseases may affect isolated individuals, without any epidemic or endemic extension. These are sporadic cases, which in some manner preserve the germ of the disease, special influences, the nature of which we are ignorant, being requisite to give to this germ the generalization of an epidemic. These great epidemic manifestations, such as the variola of 1825, are generally somewhat sudden in their origin, rapid in their progress, and, in this sense, of spontaneous origin.—*Med. Times and Gaz.*, April 25, 1863, from *l'Union Médicale*, No. 48.

13. *Contribution to the Therapeutics of Continued Fever*.—Dr. THOMAS K. CHAMBERS presented to the Royal Medical and Chirurgical Society (April 28th, 1863), an *analysis of 214 cases of fever*:—

108 treated on "general principles."

106 treated on a uniform plan of continuous nutriment and hydrochloric acid.

The first series occurred during the six years ending September, 1857; the second series during the five and a half years ending March 31st, 1863.

*Reasons for the cases being fairly comparable*.—1. They are each a consecutive series. 2. They are spread over a considerable period of years. 3. All treated by the same physician, and under similar circumstances. 4. Diagnosed and recorded by independent registrars. 5. The equality of the cases is shown by the equality of the mean duration of their convalescence.

*Of the first series*—

of 13 entered as typhus, 3 died;

of 39 entered as typhoid, 16 died;

of 56 of doubtful type, 3 died.

Of 108, total of continued fever, 22 died.

*Of the second series*—

of 19 entered as typhus, none died;

of 48 entered as typhoid, 2 died;

of 39 of doubtful type, 2 died.

Of 106, total of continued fever, 4 died.

Excluding from the first series 2, and from the second 1, who died within two days of admission, and gave therefore little scope for judging of the effects of

treatment, there remains somewhat less than 1 in 5 as the death-rate under the first treatment, and less than 1 in 35 as the death-rate under the second treatment. Therefore the second method of treatment is a powerful means of preserving life.

Details of treatment were given, and some remarks made on the action of emetics.

Dr. Weber said that, fifteen years ago, when he was a student at Bonn, the usual treatment of fever was by hydrochloric acid alone, without food. Fifteen drops of the diluted acid were given four, five, or six times a day. The mortality was about sixteen or twenty per cent., the cases being typhoid. The fever was, on the whole, more violent than here. Dr. Weber thought it would have been better to have made a comparison between the treatment by hydrochloric acid and no treatment.

Dr. Murchison had listened with much interest to Dr. Chambers' communication, inasmuch as the treatment recommended closely resembled what he had followed at the Fever Hospital during the last eighteen months. The treatment of typhus and allied fevers by the mineral acids was a very old one; it had long been the favourite treatment in many parts of Europe, particularly in Germany and Sweden. At the same time, he had not that implicit faith in it which Dr. Chambers appeared to have, and he must protest against Dr. Chambers' inference, that his treatment was calculated to prevent fifteen out of every eighteen deaths from fever. Dr. Murchison had employed the mineral acids, in conjunction with abundance of fluid nutriment, and wine when indicated, in upwards of 1500 cases of fever, and although he had often seen the most marked improvement (cleaning of the tongue, &c.), follow the use of the acids, his statistical results had been much less favourable than those now announced to the Society. He was convinced that, with more extended experience, Dr. Chambers would be compelled to modify his opinion. Dr. Chambers' statistics were open to several fallacies. 1. The cases selected for comparing the results of different plans of treatment had occurred at different periods, instead of at the same time. 2. In both series the form of fever had not been determined in a large proportion of the cases, and no details had been given to enable any one to judge of their severity. Many of the cases "of doubtful type" had probably been examples of simple fever, which was rarely fatal under any method of treatment. The results would be materially affected by the proportion of cases of simple fever or febricula in either series. 3. The rate of mortality in the first series, treated "on general principles," was far above the average mortality from fever in general hospitals, and hence it was not a fair standard of comparison. The total mortality in this series (including febricula) had been twenty per cent., for typhus alone twenty-three per cent., and for enteric fever no less than forty-one per cent. 4. The ages of the patients suffering from the different forms of fever had not been given. Age exercised little or no influence over the rate of mortality of enteric fever; but in the case of typhus, the results of different methods of treatment could never be satisfactorily compared without taking the ages of the patients into consideration. Under twenty years of age typhus was rarely fatal; above fifty, the mortality was nearly sixty per cent. 5. The number of cases was too small to warrant any decided opinion as to the advantages of the treatment recommended. The second series included only nineteen cases of typhus and forty-eight of enteric fever, diagnosed as such. A practitioner, with extensive experience in fever, might often have under his care twenty cases of typhus in succession, without losing a single case, but then if he lost five cases, the mortality would be twenty per cent. During last autumn, of forty-one successive cases of enteric fever, under Dr. Murchison's care, only two died, one from perforation of the bowel, and the other from acute tuberculosis—lesions not likely to have been cured by any treatment—yet the rate of mortality for the entire year had been considerably greater. Most of the forty-one cases had been severe, but in none had a drop of hydrochloric acid been administered. Dr. Murchison doubted if the hydrochloric acid possessed advantages over the other mineral acids. Although he believed that the treatment of continued fevers by the mineral acids, together with a uniform system of nourishment, was justified by our knowledge

of the pathology of fever, as well as by experience, he was confident that Dr. Chambers' statements were calculated to make those who heard them too sanguine as to the results to be obtained from it.

Dr. Chambers, in reply, said that all statistics were open to the objections raised by Dr. Murchison—that they were not long enough. His statistics were not brought forward as conclusive, but as a help to further inquiry; and he would grant that it might be found that the mortality was slightly different if further series of cases were compared, but not so as to affect the practical conclusion. His reason for bringing forward the two series of cases, the subjects of the paper, was, that being equal in number, and being pretty much under the same circumstances, they were fairly comparable. The difference in the mortality might be due, it was said, simply to a general difference in the mortality of the fever, at the two quinquennial periods; but he had found that the mortality from fever in the two periods was as nearly as possible equal, taking Dr. Murchison's statistics as his authority. He did not bring forward the cases to exemplify the acid treatment, but rather as evidence of the value of continuous nutriment. In fact, he believed that the acid was beneficial principally in a subsidiary manner—in preparing the digestive mucous surface for the nutriment. That it did thus produce a beneficial change was evident from the clearing of the tongue. In reply to Dr. Waters, the author said that wine was given in both classes, but only in cases in which it was strongly required, on account of the expense. The treatment in Germany by acids was not successful from being relied on solely, and not supplemented by nutriment.—*Lancet*, May 16, 1863.

14. *Fevers of the South-east Coast of Africa.* By CHAS. J. MELLER, medical officer of Dr. Livingstone's exploring party.—Although the time spent in the Rovuma, Zambesi, and Shiré rivers, has been too short to enable one to form statistics, or tabulate results of practice, I am able, from the number of cases we have had, to select the more salient points of the fever common to all the rivers; and to point out the principles of treatment in the typical form and varieties. To know the fever in its different forms, it would be necessary to study it in the reputedly healthy and unhealthy parts of the river, at different times of the year. This we have not been able to do.

We were only in the Rovuma a short time—March, and part of February, 1861—and left it, from finding it rapidly falling. Probably, we entered it just at the end of the rainy season. Before leaving it we lay by a mangrove swamp, for five days, procuring wood. A few cases of simple fever had occurred, attributable rather, I think, to exposure to the sun in boats than to malaria; but whilst we were lying by these mangroves, a more severe form rapidly spread amongst us. The patients were first attacked with griping and vomiting; followed by headache, hot skin, and the usual symptoms of the second stage; or by exhaustion to syncope, long continued rigors, or profuse sweating, without cold or hot stage. The system seemed to have suddenly received a poison of such sedative power that partial collapse ensued, reaction from which was, in two or three cases, procured only after stimulants had been frequently administered. Griping being an unusual concomitant, we looked for some cause to account for it; and thought we had found it, on observing that the water flowing by the ship, and which we had been drinking, came from a creek in the mangroves, and was exceedingly impure from the amount of vegetable matter floating on, and held in suspension in it. Within the five days, but four out of the whole number of white men on board (twenty) escaped. To avoid further infection and bad consequences, we left the river as quickly as possible, carrying, however, so much fever with us that all the sailors but one remained in the sick-list incompetent for duty for nearly a fortnight afterwards; and there were but two or three who had returned to duty when we reached Johanna on the 8th of April. An incubatory process must have existed after leaving the river; for several who were not affected severely while in it, were great sufferers at Johanna, and on the way to the Zambesi, which we reached on May 1st, having left Johanna April 22d.

The following three months, which are reputedly the healthiest, were spent in the Zambesi and Shiré; and the mild character of the fever we had would